Responses to reviewers' comments not covered by Gerrodette, Wade, Fiedler or Curry.

MMC Letter of 8 January 1999

p. 1 last par.: Rmax was estimated from the data for each stock rather than using a default value based on analogy with other species. We think this is a preferable approach.

P. 3 par. 3: We agree there is merit in reviewing information concerning the frequency with which sets have been made on coastal spotted dolphins to get an indication of the gross impact on this stock from repeated chase and encirclement. However, as discussed at the December 16-17, 1998 consultation meeting, it is not likely this will lead to major advances in our understanding. An already published study by Edwards and Perkins (1998?) estimated average annual frequencies of sets made on schools of various sizes, but we can not now estimate how this translates to frequencies for individual dolphins. This is because we have almost no information on coherence of schools over time, and without such information it is not possible to translate the frequency of sets on "large" schools to the number of times an individual is chased and encircled. For the 2002 finding we will at least estimate the average annual frequency of sets on spotted dolphins within the coastal stock's range.

IATTC Letter of 14 January 1999

Most of the comments in this letter are addressed in responses by Drs. Goodman and Wade. The points not addressed by them include the following.

P.3 par.1 item d) "lack of definitive cause and effect from planned stress and genetic studies".

This has not been denied in any written or verbal communication from SWFSC. In fact, the research report for the initial finding does not attempt to attribute causality to any source for the observed failure to recover. Rather, it considers (to the extent possible given the time allowed) information on the two most likely causes - the purse seine fishery and changes in the environment. For the initial finding it has only been asserted that it the information available does not support dismissing the fishery as a plausible cause.

P.3 par.1 item e) "ecosystem effects of large changes in environment, and abundance of tunas and other species over the period of data collection and over the recent period of interest."

We agree that this possibility deserves study, and we communicated this verbally at the 16-17 December 1998 meeting participated in by IATTC staff. Our report also states that we acknowledge the possible role of ecosystem effects in any failure to recover. It however also states that the environmental variables examined do not show evidence of a decadal-scale environmental shift. We will be looking carefully at data on a number of other upper-level predators and on a few data sources on prey for the 2002 finding. Analyzing this type of data for evidence of community-level ecological changes is a major, complex and time consuming task. It certainly is not one that is feasible for this initial finding.

The common implication of this item and item a) is that tunas and billfishes have in some way replaced dolphins ecologically such that the carrying capacity for the mammals has been reduced. This is an interesting line of speculation that potentially could be developed into a set of scientific hypotheses. For the 2002 report we will examine available data sources on birds and large fishes for evidence of shifts in community composition during the past decade. However, even if such shifts are observed it will still be less-than-straightforward to connect them to the dolphin stocks' failure to recover. Some concerns that immediately come to mind include i) while there is some very limited evidence of shared prey types among tunas, dolphins and billfishes, there is no indication that they are competitors. The simple occurrence of shared prey types among a long menu of types consumed is generally not accepted as evidence of competition (many literature references can be provided if needed, but consultation of any recent ecological text book should suffice). Reduction of dolphin numbers does not obviously or necessarily create opportunity for tunas. It would have to be demonstrated that there was (among other things) a shared resource that was limiting population growth. Such a demonstration seems a daunting task. ii) the major stocks of large tunas in the region have also been reduced by fishing pressure. Closure of the Commission Yellowfin Regulatory Area in 1998 suggests conservation problems exist for this species too. This isn't consistent with a population that has grown to a new, higher level in the absence of previously-abundant dolphins, and is now hindering their recovery.

P.3 par.1 item f) "bias resulting from literal interpretation of the fishery-based tuna vessel observer data."

We have interpreted these data no more literally than the IATTC in its many published papers on the data, and the resulting estimates of relative abundance and their use to track trends over time. Please see Appendix 1 of the report which reviews this literature. We do acknowledge that these estimates are less than ideal, but the peer-reviewed published accounts consistently assert the validity of the estimates for monitoring trends. Only one published paper contradict this, by Edwards and Kleiber (1989. Effects of nonrandomness on line transect estimates of dolphin school abundance. Fish. Bull. 87(4): 859-876). This simulation study dealt with possible biases in abundance and trend estimates resulting from varying levels of school clustering across years. A subsequent paper by Buckland et al. (1992. Estimating abundance of tuna-associated dolphin stocks in the eastern tropical Pacific. Fish. Bull.90(1): 1-12) specifically countered this argument, and concluded that the TVOD estimates were in fact good indicators of abundance trends. In the absence of more recent, peer-reviewed science demonstrating the contrary, we feel the only appropriate course is to follow precedent and include the TVOD indices in our assessments (as was done in the earlier assessments by Wade that resulted in the current designation of depleted status under the MMPA for northeast offshore spotted and eastern spinner dolphins). On the contrary, we would have only subjective or unpublished assertions to use as a basis to exclude them.

P. 4 par 1. Our environmental analyses did in fact consider variability in the years between 1988 and 1998 (see Fiedler responses and paper for more details).

P. 4 par 3. This paragraph asserts one of a number of possible interpretations of the apparent gap

in age distribution for spotted dolphins in the kill statistics. Please see attached memo from Bill Perrin to Steve Reilly which addresses this subject in somewhat more depth. As Perrin points out, the gap existed only for females, and was most likely the result of an operational bias in processes that result in kill and sampling, possibly due to variation in vulnerability by age and sex. He further notes that the liklihood is low of there being separate schools of juvenile females that somehow consistently are not recorded by the observers. He notes that while seiners don't often set on small schools of spotted dolphins, they do look them over for signs of accompanying tuna, and the observer does log them when they are seen. It seems highly unlikely that, if such schools of juvenile females existed, they would have been distributed in areas not traversed by the tunaboats or totally ignored by the fishermen and the observers.

Further, within recollection of observers who have made years' worth of trips to the ETP to search for dolphins, NMFS research vessel sighting surveys have not recorded such schools composed of juvenile spotted dolphins (either alone or in combination with other species). We are, however, examining our files for such events, and also looking into a large series of vertical aerial photographs to pursue the possibility that these putative juvenile schools were seen but not identified as such by the shipboard observers using 25x binoculars, but were only identifiable from the air.

P. 4 par 4. This is an interesting assertion, and we look forward to seeing a completed analysis that has undergone peer-review.

IATTC Letter of 3 February, 1999

All comments dealt with by responses of Fiedler and Curry.

IATTC Letter of 17 February, 1999

Replies by Drs Goodman and Wade address all comments here with the partial exception of the use of IATTC abundance indices already addressed (please see comments above to IATTC letter of 14 January 1999, P.3 par.1 item f), and the closing paragraph asserting that, "...the proposed decision analysis is so flawed that I recommend the NMFS abandon it..." To that point we respectfully reply that the report by Dr Goodman and his replies to the detailed comments in this and the earlier letter do support the use of this approach. To this we would add the suggestion that Dr Allen and the IATTC staff read and consider the very positive comments of the Marine Mammal Commission (letter from J. Twiss, 12 February 1999) on the decision analysis framework.